AN

INAUGURAL ESSAY

ON

DROPSY,

OR THE

HYDROPIC STATE OF FEVER.

SUBMITTED TO THE

EXAMINATION

OF THE

REV. JOHN EWING, S. T. P. *Provost*,

THE

TRUSTEES

AND

MEDICAL FACULTY

OF THE

UNIVERSITY OF PENNSYLVANIA,

ON THE TWELFTH DAY OF MAY, 1797.

FOR THE

DEGREE

OF

DOCTOR OF MEDICINE.

BY WILLIAM ALLSTON,

OF GEORGE-TOWN, SOUTH-CAROLINA,

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TO

Robert Brownfield, A. M. Phyſician,

GEORGETOWN, SOUTH-CAROLINA.

MUCH RESPECTED SIR,

ACCEPT as a tribute of gratitude and eſteem, the firſt fruits of that ſtudy which commenced under your patronage. And be aſſured, that a lively recollection of the many favors received from you, can only be terminated with the life of,

Reſpected Sir,

Your very ſincere friend,

And ever affectionate pupil,

WILLIAM ALLSTON.

May 3d, 1797.

TO

BENJAMIN RUSH, M. D.

PROFESSOR OF THE

Institutes and Clinical Medicine,

IN THE

UNIVERSITY OF PENNSYLVANIA.

HONORED SIR,

TO whom with more propriety can the following Dissertation be dedicated, than to him whose principles can be traced in every page of it?— Receive then this my first attempt, as a mark of that respect I owe to your illustrious character.

Allow me, Sir, to make you my sincere acknowledgments for the invaluable discoveries and improvements you have made in medicine. And suffer me in this manner to present to you my unfeigned and most cordial thanks for your constant politeness and friendship, ever since I have had the honour of residing with your amiable family. And believe me, when I assure you, Sir, that no possible change of my situation can ever lessen that real affection which is now so fervent in the bosom of,

My honoured Sir,

Your affectionate friend,

And grateful pupil,

WILLIAM ALLSTON.

May 3d, 1797.

DROPSY.

WHEN the catalogue of diſeaſes is examined, it will be evident, that of all which attack life, no one appears to be more conſtantly fatal in its termination than that which is called DROPSY. Senſible of this, the Author of the following DISSERTATION, having no other motive than a compliance with the laws of this Univerſity, and an earneſt deſire to alleviate the ſufferings of his fellow-creatures, hopes it will eſcape the eye of criticiſm, and that every allowance will be made for this incorrect and youthful performance.

DEFINITION.

DROPSY is an appellation given to that diſeaſe in which there ſeems to be a preternatural collection of ſerum or water, in ſome one or more parts of the human body. A doubt has ariſen reſpecting the derivation of this name: Some have ſuppoſed it from the Greek, while others* have aſſerted it

* Wilkes, page 2.

to come from the Saxon. As the preſent intention is not to dive deeply into the minutiæ of this diſeaſe, but only to examine the general nature of it; and as ſuch a diſquiſition would at beſt be but ſpeculative, and could not in the leaſt elucidate or advance the cure, I will leave this determination for future enquirers; conſcious that although they may make it even of a different origin, they will all mean by it the ſame diſeaſe, produced by ſimilar cauſes, and whoſe method of cure will be ſtill the ſame.

This diſeaſe is ſaid to be generally known by an enlargement of ſome part of the body, accompanied by particular ſymptoms, which are, a difficult reſpiration, great thirſt, ſcantineſs of urine, and ſwelled ancles. Others are ſaid to occur frequently, though not conſtantly; as, a pain in the thorax, cough. As the four former are ſaid always to be not only preſent, but ſtrictly Pathognomonic, it cannot be amiſs here to examine if this be really the caſe; if it be not, then the impropriety muſt appear of placing confidence in any ſymptom as being characteriſtic of one diſeaſe alone.—And firſt of

DIFFICULT RESPIRATION.

THIS has long ago been conſidered as a ſymptom inſeparable from this diſeaſe; Dr. Sydenham*

* Wallis's, page 262.

ranked it as a conſtant one. With all the deference due his great character, I muſt beg leave here to diſſent from him in opinion; for although it may, and very frequently does exiſt, yet it cannot be imagined always to be preſent; for it is now very certain, that there never was a ſymptom which conſtantly accompanied any diſeaſe. The yellow fever, for inſtance, was ſuppoſed by many to have invariably that tinge from which it took its name; yet Drs. Moſely and Ruſh, and the too fatal experience of 1793, have taught the contrary. The error of Drs. Sydenham, Monro* and others, muſt have been the reſult of a falſe theory, ſuppoſing it the effect of the preſſure of the water upon the diaphragm, and the contents of the abdomen. This opinion muſt at once crumble to its original ſtate, when from experience it has been found that this ſame ſymptom not only exiſted, but was equally as diſtreſſing in thoſe diſeaſes in which, from diſſections, there was not diſcovered the ſmalleſt quantity of water; but on the contrary, evident marks of inflammation; as in thoſe who have died from a pleuriſy, the meaſles, or ſmall-pox. Now ſurely no one will advance, that this ſymptom is characteriſtic of either of theſe diſeaſes, as diſtinct from the others; for it exiſts in all: And if it is not, why ſhould it be ſuppoſed ſo in the Dropſy? I muſt confeſs I know not. But this impropriety will be ſtill more glaring when it is diſ-

* Monro, page 9.

covered, that it does not always take place in this diſeaſe; and in proof of this, we are aſſured by Dr. D. Monro* that his experience taught him there had been many dropſies in which this ſymptom did not exiſt. And if this ſymptom does not exiſt at all times in this diſeaſe, and is as often in other diſeaſes, now ſuppoſed eſſentially different therefrom, might it not be accounted as much pathognomonic of the latter as of the former? And if it be a ſymptom ſo common in moſt ſtates of fever as experienced authors have recorded, and in all diſeaſes primarily of the blood veſſels, may it not, even in the dropſy, be attributed to an irregular or convulſive action of the arterial ſyſtem?

THIRST,

Or the next ſymptom for our preſent conſideration, and which Dr. Sydenham† declares is one of the three abſolutely neceſſary to conſtitute this diſeaſe. It cannot be denied, that this ſymptom attends as commonly, and is as diſtreſſing as any which occurs: but nevertheleſs, that it ſhould here be conſidered as pathognomonic is not altogether ſo admiſſible; for it is very common in every ſtate of fever as well as in the Dropſy. But that it cannot be characteriſtic of this diſeaſe, it has ac-

* Monro, page 9.
† Wallis's, page 262, vol. ii.

cording to Dr. Darwin* been found wanting. This fact makes it, from the great analogy with the other ſtates of fever, very probable; for in them there are ſtates, as experience muſt have witneſſed, even of the moſt inflammatory grade, in which this familiar ſymptom is frequently wanting. And if it be in this diſeaſe as in fever, may it not be doubted whether it be pathognomonic of the former; but rather, as in the latter, the entire effect of a morbid action of the blood veſſels?

DIMINISHED SECRETION OF URINE

Is the third and next pathognomonic ſymptom which has been noticed from the earlieſt ages of medicine to the preſent period. Vanhelmont, as abſurd in this diſeaſe as in many others, aſſures us, that no man whoſe urine paſſes freely, will ever become dropſical; and to this day it is a common direction, to obſerve its quantity. So far however from this aſſertion being grounded upon experience, Wallis,† Wilkes, and others affirm, there have been caſes of this diſeaſe, in which no ſuch ſymptom exiſted. And Dr. Leigh ſays‡ there have been dropſical patients who, through the whole courſe of the diſeaſe, had there urine both in quantity and quality. That it is proper to pay attention to this diſcharge when we adminiſter re-

* Zoonomia, page 314, vol. i.
† In his tranſlation of Sydenham, page 262, vol. ii.
‡ Nat. Hiſt. Cheſhire, page 69.

medies to promote it, no one will deny ; but that it ſhould be ſo narrowly watched, and even ſuppoſed the cauſe of this diſeaſe, muſt be the remains of ignorance and ſuperſtition. As well might we imagine the vomiting in a cholera morbus, or the diſcharge of ſyballa in the dyſentery the cauſes of thoſe diſeaſes, when in reality they are an effect. Beſides, this ſymptom has been found in the different ſtates of fever, as in the eruptive; for Sydenham long ago in the ſmall-pox cured this ſymptom only by expoſing his patient to the cold air: and in the bilious remittent ſtate Dr. Ruſh informs us in his account of that fever in 1793, that he often met with it.—From what has been advanced on this ſubject, I cannot by any means admit this ſymptom as pathognomonic of the Dropſy more than of the other ſtates, but ſhall diſmiſs it with aſſerting it to be, as in the other ſtates of fever, the effect of an irregular action of the arterial ſyſtem.

SWELLED ANCLES.

That ſwelled ancles have ever been conſidered as a ſymptom characteriſtic of this diſeaſe, all ancient authors will evidently prove; and Dr. Sydenham* places it as an infallible one. From this ſanction of antiquity, modern phyſicians ſcarcely ever once loſe ſight of it, and ſeldom or never do they attempt to preſcribe for hydropic patients, without firſt enquiring if this exiſt, and if ſo, then the di-

* Page 262, vol. ii.

ſeaſe is evident, and their judgment at once confirmed. But notwithſtanding the univerſal prevalence of this opinion, I hope to be excuſed for not ſubſcribing to it; and that too, becauſe from the writings of Dr. Sydenham* it appears that he himſelf found it erroneous: hence ſhortly after he adds, "it does not always hold good, for it is often in pregnancy, and frequently in obſtructed menſes. It is not only an uncertain one in women, but alſo in men, being often in them the conſequence of aſthma." Here it muſt be certain the Doctor found this ſymptom in diſeaſes which his principles could not at any rate admit as ſimilar: how therefore could it prevail in diſeaſes ſo widely different, and yet be pathognomonic of the one and not of the other?—This difficulty will be ſurmounted when it be proved that Dropſy is as much a diſeaſe of the arterial ſyſtem as either obſtructed catamenia or the aſthma; for then it will be plain, that in either caſe it is the action of ſimilar cauſes upon the ſame ſyſtem. But, that in all probability it is not a pathognomonic ſymptom, Dr. Wilkes† poſitively aſſerts, that there have been inſtances of this diſeaſe which terminated fatally, and in which the legs never once ſwelled. If this ſymptom alſo be not at all times connected with the dropſy, and exiſt in moſt ſtates of fever; have we better authority to make it characteriſtic of the one than of the other? And if there be not, may I not conclude, that in every

* Page 263, vol. ii.
† Page 44.

caſe, whether ariſing in dropſy, obſtructed catamenia, ſcarlatina, or what it may, it is always the effect of an irregular or convulſive action of the arterial ſyſtem?—It may here be aſked, if I admit any of theſe ſymptoms as pathognomonic? The anſwer is in the negative; for I have already proved that they are all exceptionable, being abſent in ſome dropſies and preſent in other diſeaſes which very few or none will acknowledge of a ſimilar nature. The utility of a proper theory, however ridiculed by many, can never perhaps appear in a more ſtriking and ſatisfactory point of view than in the preſent diſeaſe; for when we admit dropſy to be a genuine fever, as I hope to prove, we at once deſtroy thoſe innummerable contradictions, and every ſymptom is ſeen in its original ſimplicity, and muſt inſtantly be acknowledged the effect of the ſame cauſe. Then it will be readily admitted, that a difficult reſpiration is the effect of that irregular action of the veſſels by which means the blood is propelled through the lungs with ſuch impetuoſity as to give riſe to laborious breathing. Neither is this action always of an equal ſtrength, but muſt alternate according to the quality and duration of the exciting cauſe; and hence the variety in this reſpiration. Thirſt and paucity of urine may alſo be explained by admitting, that from this morbid action the blood is haſtened in larger quantities than uſual into the different glands; in conſequence of which they are ſo choaked that the ſecretory veſſels cannot perform their office. And this is in-

deed probable, becauſe the kidneys have often upon diſſections been found to have laboured under an engorgement; and becauſe cold and the common evacuants are effectual in reſtoring theſe ſecretions. Nor will this action of the veſſels forſake me, when I examine the ſwelled ancles; for it has already been urged, that an effuſion is the entire effect of it; cauſing the exhalants to throw out more than was natural. In doing which, from their exceſſive force the action of the blood veſſels, if I may be allowed the expreſſion, is raiſed or increaſed from the healthy or natural point*, to the effuſive. In ſlighter caſes,

* It muſt here be noticed, that the inſtitution of theſe points promiſes the greateſt utility to the world, and for which it muſt ever be indebted to the Profeſſor of the Inſtitutes of Medicine in this Univerſity. When a perſon is in good health he is at the *healthy point*, the firſt grade above which is the *point of ſimple morbid action*; this is the firſt change from common health, and in it the arteries are alone diſeaſed without the leaſt local affection. When this action is more violent a congeſtion in ſome part takes place, and hence a pain; this may be called the *point of congeſtion*, or rather, the *inflammatory point*: and if the ſyſtem is raiſed higher, an effuſion is either the conſequence, and hence the *effuſive point*, or a ſuppuration, which forms the *ſuppurative point*. And if the action of theſe veſſels is yet increaſed to an higher pitch, a mortification takes place, and hence the *gangrenous point*. And either of theſe may exiſt without the others: for it has often been noticed that fevers have prevailed without any other affection than a ſimple morbid action of the blood veſſels, while other ſtates of it have an inflammatory affection from the commencement. Some have two or more without the others, as rheumatiſm the inflammatory and effuſive points without the ſuppurative, and the ſmall pox the inflammatory and ſuppurative without the effuſive; and mortification

as of obſtructed catamenia, or gravidation, in conſequence of prediſpoſing debility from much walking, the effuſion is thrown altogether upon the lower extremities; but in thoſe more violent, from the inteſtines being a more delicate part, the effuſion is formed altogether in the abdomen; and in the moſt ſevere, every part of the whole body is liable to it. —If ſuch then be the ſtatement of the proper nature of theſe ſymptoms, they cannot be viewed in any other light than as the effect of an irregular action of the arterial ſyſtem, which may at all times be diſcovered by a proper attention to the pulſe or ſtate of the ſyſtem.

DIAGNOSIS.

AS the hiſtory of medicine abounds with innumerable melancholy inſtances of pregnant women being by the moſt ſkillful not only imagined dropſical, but have even been operated upon; and as many who really laboured under that diſeaſe, have been ſuffered to linger in torture; it muſt readily be acknowledged, that all theſe boaſted criterions are but fallacious, and that the Diagnoſis has at all times been uncertain. Under this head I ſhall only obſerve, that when this diſeaſe is conſidered as a fever, it will always be known by the irregular action of the arterial ſyſtem.

the inflammatory and gangrenous without the effuſive: And frequently in dropſy the effuſive exiſts without either the inflammatory or the gangrenous.

As the

PROGNOSIS

Of a diſeaſe is often of the greateſt conſequence, not only to remove the apprehenſions of the friends, but alſo to ſecure the character of a phyſician; it would at all times be an object to obtain ſuch information as would enable an immediate and favourable prediction. In this diſeaſe however, as nothing certain has ever been diſcovered, and as the ſymptoms are ſo very changeable, it muſt be admitted that the prognoſis even here is to be governed entirely by the ſtate of the ſyſtem.

In order to make a more complete analogy between this diſeaſe and fever in general, it will be proper to examine for a few moments only, ſome of the different theories of this diſeaſe. And firſt, that which places it in the

LYMPHATIC SYSTEM.

THE diſcovery of the Lymphatics opened a wide field for ſpeculation in medicine; and of all the diſeaſes incident to man, no one has been ſo conſtantly ranked in, or implicitly believed really to be of, that ſyſtem, than that under our preſent conſideration; it of courſe therefore will be proper firſt to enquire in what manner this is ſuppoſed to take place.

From diſſections exhibiting the effuſed water in two forms, viz. in cyſts and diffuſed; it was ſup-

poſed to be the conſequence either of a dilatation or rupture of theſe veſſels. That the former of theſe cannot be the caſe, is adduced from the obſervations of Dr. Monro, who aſſerts* that there has not been a ſingle inſtance in which this dilatation followed the known courſe of theſe veſſels. It is now however well eſtabliſhed that when theſe are affected by a ptyaliſm or the venereal diſeaſe, their courſe may eaſily be followed, in conſequence of their enlargement. But theſe cyſts have been diſcovered the ſize of an orange, which is as large, according to Monro† as the greateſt known aorta without a rupture, and which in its natural ſize was not only three hundred times bigger than any lymphatic, but whoſe coats were as proportionably ſtrong. Surely then no one can pretend to ſuppoſe theſe delicate veſſels would remain unruptured when ſo preternaturally diſtended. But theſe veſicles have been diſcovered in the abdomen entirely unconnected with any thing, only floating in the effuſed liquor. If theſe were dilated ſections of theſe divided veſſels, might not the ſpot be detected from whence they were detached? But even admitting that a rupture does take place, which no one can believe, it will be enquired, what are the cauſes of this ſudden dilatation; and if this is ſaid to be a ſtimulus, it muſt be recollected that the action of this is primarily upon the arterial ſyſtem.

* Monro on the Dropſy, page 20.

† Page 21.

THE rupture of theſe veſſels has for a length of time been eſteemed a fruitful ſource of this diſeaſe; indeed the fatal experience of many authors proves moſt incontrovertibly that in it a rupture does ſometimes exiſt: But that this ſhould therefore be imagined its origin or immediate cauſe, ought not, in my opinion, to follow; for there are many proofs that in moſt caſes of this diſeaſe a rupture never was diſcovered; and there are alſo numberleſs inſtances of theſe veſſels being ruptured and divided, in which no ſuch conſequence as dropſy ever was noticed. This, Lower aſſures* us, he has often ſeen, and the experience of every phyſician muſt have witneſſed the ſame. But that a rupture is not a very frequent attendant, and of courſe cannot often be a cauſe, Monro, Hewſon and Cruickſhank all unite in affirming that the muſcular fibres of theſe veſſels, though delicate in ſtructure, are notwithſtanding very ſtrong. But if a rupture is allowed, it muſt next be enquired, what are the names of thoſe boaſted remedies which to effect a cure, muſt act as vulnerary? And if there are not any, it muſt be concluded that a rupture cannot be a cauſe; for with it the operation for the Paracenteſis can never promiſe ſucceſs, it being only evacuating one quantity of water to make room for another further and greater diſcharge.

IN Dr. Withering's remarks upon the Dropſy† "he ſuſpects that many of them originate from pa-

* Cap. ii. page 29.
† Recorded in the Medical Com. vol. v. page 374.

ralytical affections of the lymphatic abſorbents;" and from which poſſibly Dr. Darwin in the firſt volume of his Zoonomia, has taken the theory, which he has ſo ſtrenuouſly attempted to ſupport. But after all the exertions of his great genius, I muſt be permitted to differ from him in opinion, and that too in conſequence of his plauſible theory not ſtanding the teſt of an examination. By a paralyſis is always meant a loſs of voluntary motion in any part in conſequence of either a compreſſion of the brain or origin of the nerves, or upon ſome other part preventing the proper motion of the ſaid nervous fluid. But ſays Dr. Darwin* "a paralyſis has been uſed to expreſs the loſs of voluntary motion; but may with equal propriety be applied to expreſs the diſobediency of the muſcular fibres to the other kinds of ſtimulus." It muſt be evident to every one that his definition cannot be admitted; for it is now a very familiar circumſtance to notice muſcular fibres under ſuch violent action as not only to be diſobedient to other ſtimulants, but entirely inſenſible to all, and yet not labour under a paralyſis: this is ſeen after ſevere exerciſe, as in fatigue: alſo in violent ſpaſmodic conſtrictions of the muſcles and inteſtines, and ſurely in neither of theſe can it be advanced that a paralyſis has the ſlighteſt tendency to take place.

Again, when the cauſe of this ſaid paralyſis is noticed, it muſt at once be confeſſed, that ſuch a

* Sect. 28, page 297, vol. i.

thing, in all human probability does not occur. This, Dr. Darwin aſſures us* is a violent ſtimulus. It is true, that when a ſtimulus is received into the ſyſtem it muſt be conveyed through the abſorbents; but that its effect muſt of courſe be upon them is not either from demonſtration or reaſon the conſequence: for diſſections have never warranted this concluſion, and reaſon would lead us directly to the contrary: for when it is recollected that its paſſage muſt not only be ſhort, but its motion muſt be in us very rapid, being, according to Cruickſhank† in a dog twenty feet a minute, it cannot but be acknowledged that its effects ought not to be ſought for in this ſyſtem. For if it is borne in mind how ſoon it is emptied into the blood veſſels, how conſtantly it circulates through every one, how abſolutely neceſſary this is both for the ſupport of life and the proper action of even the abſorbent ſyſtem itſelf, and how invariably its effects are diſcovered primarily in the blood veſſels; it muſt be granted, that a paralyſis of the abſorbents in the dropſy is hypothetical, and that this diſeaſe is the effect of ſtimulus acting primarily upon the arterial ſyſtem.

Another, and as yet more prevalent opinion is, that a diminiſhed abſorption, and that too from a loſs of tone in the abſorbents, is an immediate cauſe

* Page 297.

† Page 38, chap. v.

of this diſeaſe. That theſe veſſels are at times not able to take up all the effuſed fluids, and have, as advanced by many, even remained for months in them, being as it were inſenſible to their action, cannot be denied: But that this ſhould depend upon an abſtraction or want of tone from the commencement, does not follow; for there are not any evidences of it, aſſertions only having been advanced, and all the attributed cauſes tending rather to increaſe than to diminiſh or weaken it. Obſtructions in them and their glands, have been by Dr. Cullen and others, ſuppoſed of the number. I cannot however ſuppoſe that theſe produce the diſeaſe, for it muſt appear from diſſections that it has often occurred without the leaſt ſymptoms of them, and that they often exiſt without producing this diſeaſe; for Dr. Cullen, although a great friend and advocate for this opinion, candidly acknowledged* that he had ſeen ſeveral inſtances of the moſt part of the meſenteric glands being conſiderably tumified, without interrupting the tranſmiſſion of fluids to the blood veſſels, or occaſioning a dropſy, and if ſo, they certainly cannot properly be a cauſe of the diſeaſe. For if they do occur, muſt they not be the conſequence of, either an increaſed velocity of the fluid in them, or of the veſſels appropriated for their nouriſhment; elſe of the increaſed action of the muſcular fibres of both ſetts of veſſels? If either of theſe be admitted, then it muſt follow,

* Page 274, vol. iv.

that ſome ſtimulus a tergo exiſts, and on which this increaſed action depends; and when that is granted, it cannot any longer be urged that they labour under a loſs of tone or direct weakneſs, for it muſt be poſitive that their tone by this ſtimulus cannot but be increaſed. But to place it beyond a poſſibility of doubt, that this diſeaſe is not the conſequence of a loſs of tone in the abſorbent ſyſtem, an extract from the manuſcript notes taken in 1786, of the clinical lectures of the celebrated Mr. Cline of London, and obligingly preſented for peruſal by my friend and fellow-graduate, Mr. Stock, may not be amiſs. In lecture fourth, when ſpeaking of the diſeaſes of the abſorbents, Mr. Cline thinks "their debilitated ſtate ſeldom gives riſe to dropſy; for in that diſeaſe they generally appear *larger*, and convey a greater quantity than in health." With ſuch proof I ſhall not heſitate to aſſert, that in this diſeaſe theſe veſſels do not labour primarily under a loſs of tone, nor are even affected, and that when they are diſeaſed it is in conſequence of indirect debility ariſing from their exceſſive action. This at once ſolves the difficulty of moſt authors, why the abſorbents remain in a body of fluid for months apparently inactive; for only remove a part of this water, or reduce the quantity of ſtimulus, by any means; you will take off a part of the load under which they laboured, and thereby they will renew their natural and healthy action, and certainly do their part in removing the remaining effuſion: and that this is really the caſe the remedies which

effect a cure ſeem to prove; for they are not thoſe which grace the liſt of tonics, but ſimply evacuants, as emetics, cathartics, ven. ſection, and the operation for the paracenteſis. From what has been advanced, I preſume it is tolerably well eſtabliſhed that dropſy does not in common ariſe in any of the different ways juſt inveſtigated; and if not, there certainly cannot be the ſmalleſt ſhadow of probability for ſuppoſing this diſeaſe the continual offspring of a diſeaſe of the abſorbent or lymphatic ſyſtem. But when I conſider every circumſtance carefully, and examine every phenomenon impartially, I cannot even heſitate to decide at once with Dr. Ruſh and Mr. Cline of London, that "dropſy ſeems rather to ariſe from a greater quantity being poured forth by the arteries, than from a defect in the abſorbents."*

LAXITY OF THE FIBRES.

THE impoſſibility of accounting for many phenomena of this diſeaſe, if of the lymphatic ſyſtem, induced Dr. Donald Monro to advance the laxity of the fibres as the moſt frequent cauſe; and which he ſays† may be produced by great evacuations, a ſedentary life, or watery diet. It muſt here be evident from theſe cauſes, that Dr. Monro did not li-

* Cline's Lecture IV.
† Page 9.

mit his laxity to any particular part of the body, but had a conſtant reference to the whole ſyſtem; and as no one will deny, but at times ſuch an apparent laxity does exiſt, it will be proper to examine its nature before it can be determined if it be the immediate cauſe of this diſeaſe.—By a laxity of the fibres is meant that general relaxation or want of a healthy contraction, the entire effect of an abſtraction of a ſtimulus which was neceſſary for the health of the perſon; and which, when again renewed, produces that ſtate. Famine produces a laxity of the fibres, in conſequence of food, a natural and healthy ſtimulus, being withheld; afford this in its proper quantity and it will produce a ſimilar ſtate of contraction or health. The ſtrings of a violin when unſcrewed labour under a ſtate of laxity, becauſe the ſtimulus of the ſcrews for their ſonorous or proper ſtate of tenſion is abſtracted; apply this ſtimulus however again in its particular quantity, and their healthy ſtate or proper tone can be perceived:—preciſely ſuch ſhould it be with the fibres of the body. But that this is the ſtate of the fibres, and the immediate cauſe of this diſeaſe, do not appear; for if the cauſes were able to produce it, they ſhould do it when applied in the healthy ſtate, for only in that ſituation can they be ſaid to produce a laxity of the fibres. A ſedentary life has appeared to be a fruitful ſource of this diſeaſe, but how far it is in itſelf ſo, is not certain, for it ſeems that many people

have for years endured it, without having once exhibited the leaſt ſymptom of this diſeaſe. Now were this an immediate cauſe, why ſhould it not have produced it in theſe many inſtances, and when too a laxity might be expected? for by their action they would reduce the ſyſtem below the healthy point, and thus have produced it. But ſay ſome, it has been found to happen. No one will deny this; but never while the ſyſtem was in that ſtate, nor before ſtimulants were improperly uſed to give, as it has been frequently called ſtrength. For example, let a perſon who from a ſedentary occupation takes but very little or no exerciſe, call in the uſe of ardent ſpirits as a ſubſtitute for it, and he will in a ſhort time find himſelf dropſical. Whereas, had he taken leſs aliment, and laid aſide thoſe baneful liquids, he would, even with the ſame exerciſe, have enjoyed his perfect health. This may be accounted for in the following manner: in the former, he enjoyed tolerable health, though his exerciſe was trifling; but ſuppoſing a want of that proper quantity previouſly accuſtomed to might affect his health, he hopes to obviate it by the uſe of more aliment and ardent ſpirits; when in fact he produces the very evil he wiſhed to avoid; for theſe ſtimulants created an inequality of excitement and excitability, and at once raiſed the ſyſtem from the healthy to the effuſive point. But in the latter, the little exerciſe created an increaſed excitability, upon which the moderate ſtimuli acted, according to a law of the animal economy, with an increaſed force, and kept

up that equilibrium of excitement and excitability which conſtitutes health. This familiar example at once ſhews the impropriety of imagining that this mode of living creates ſuch a laxity of the fibres as is generally believed; and that an effuſion is not the effect of ſuch a laxity, but rather of that morbid excitement of the fibres of the blood veſſels, the entire conſequence of ſtimuli diſproportioned to the excitability of the ſyſtem. The other ſaid cauſes act in the ſame manner, as will be more properly noticed hereafter.

INCREASED EFFUSION.

SENSIBLE as the late great profeſſor Dr. Cullen muſt have been of the inſufficiency of either of theſe theories, when ſeparate, to ſolve the many difficulties ariſing therefrom, he ingeniouſly formed* a compound, which he thought capable of ſatisfying all objections, and of unlocking many of the myſteries of this diſeaſe. An increaſed effuſion or diminiſhed abſorption being delivered by ſo able a man, for a while carried complete conviction to the minds of all enquirers, and obliged thoſe who preferred the ſentiments of others to the humble exerciſe of their own judgments, at once implicitly to receive it as a ſpecimen of human perfection. As the latter part of the Doctor's theory has al-

* Page 250, vol. i.

ready been diſcuſſed, it will be proper here to examine if an increaſed effuſion does take place in the manner by him ſuppoſed.

It has been argued by Dr. Cullen* that an interruption to the free return of the venous blood from the extreme veſſels of the body, to the right ventricle of the heart, is a cauſe of this effuſion. That this indeed very frequently accompanies this diſeaſe no one can doubt; but that it ſhould be ſuppoſed its cauſe of courſe, is not altogether ſo rational; for with equal propriety might it be urged, that as the cold ſtage preceded the hot, or even exiſted in the ſame paroxyſm with it, it muſt be its cauſe; whereas all admit that the cauſe of the cold or firſt ſtage, is the cauſe of all that follow. Preciſely ſuch is it with obſtruction, and that it is not the proper cauſe of this diſeaſe is probable from its being itſelf the effect of a diſeaſed action of the ſyſtem; being never produced until the arterial ſyſtem has previouſly laboured under a diſeaſe. Marſh effluvia, ardent ſpirits, or fatigue, are proper cauſes of this diſeaſe, becauſe they all act upon the body when in an healthy ſtate, that is, when the excitability and excitement are equal, and muſt of courſe, either from an exceſs of quantity or an improper application of their ſtimulus, produce this diſeaſe; but an interruption to the free courſe cannot, except from mechanical preſſure, be a cauſe, for it is not a ſtimulus acting upon the body free from diſ-

* Page 252, vol. iv.

eaſe, but is itſelf the effect of an irregular action of the blood veſſels, produced by a diſproportioned ſtimulus. Neither does the diſcovery of a polypus, or an oſſification of the valves of the heart, Dr. Cullen's ſuppoſed cauſes of this interrupted circulation, invalidate my opinion in the leaſt; for if an improper uſe of theſe ſtimulants have, from an exceſſive action of the veſſels, produced an effuſion of lymph in pleuriſy and croup, a rupture of blood veſſels in apoplexy, chalk ſtones in gout, an oſſification of the longitudinal ſinus* in mania, and alter, according to Dr. Ruſh,† even the hair in drunkards: why may it not produce in the vena cava, and heart itſelf, ſuch morbid phenomena? And further that this is the caſe appears probable from a fact related by Hewſon on the lymphatics‡, and extracted from the minutes of Sir John Pringle, of a man who after having laboured under palpitations of the heart for ſome time, died apoplectic. Upon diſſection the heart was found large and adhering to the pericardium, and had marks of inflammation. In the pericardium was a ſmall quantity of bloody ſerum; the coronary arteries were oſſified, and in the

* Caſe in the Hoſpital, in 1796.

† Dr. Ruſh in his lectures, informs us, that ardent ſpirits change the tendons, arteries, and even the pleura in bony matter; and alſo affects the hair very much, making it criſp: and that the barbers in London are ſo well acquainted with the fact, that they will not give more than half price for the hair of drunkards.

‡ Page 113.

pulmonary artery and right ventricle of the heart, was a large concretion, ſuppoſed to be a polypus; the upper part was white and fizy, and the under like congealed blood. If ſuch then be the appearance from diſſection, and from which no dropſy was the conſequence; is there not a ſufficient reaſon to conclude that this interruption of the blood from the extremities to the heart is not the cauſe of this diſeaſe, but is, like a polypus and oſſification of the ventricles of the heart, the entire effect of a morbid ſtate of the blood veſſels?

Not only an interruption to the courſe of the blood through the heart and large veſſels, but alſo obſtructions of the different viſcera have been by ſome imagined a frequent cauſe. A ſimilar objection to theſe might be advanced; for in all there had pre-exiſted a morbid action of the blood veſſels, for in ninety-nine of an hundred caſes, they are the conſequence either of an obſtinate, or of an ill cured fever, which is now univerſally acknowledged, a diſeaſe of the arterial ſyſtem. Dr. Cullen* appears to have been perfectly ſatisfied that an obſtruction of the liver was a very common cauſe; and this he ſuppoſed very probable from the ſituation of the vena portarum, being ſuch that this obſtruction could not but impede the regular return of the blood. No diſſection however has as yet confirmed this plauſible opinion; for it has on the contrary been diſcovered that theſe veſſels are not only

* Page 254, vol. iv.

undiminiſhed in diameter, but much enlarged: accordingly, it may be imagined that they convey more blood than natural. This is not hypotheſis alone, for frequently this viſcus has been found ſchirrous:—now this ſtate is certainly the conſequence of inflammation; and this by all is acknowledged the effect of a morbid action of the blood veſſels. But that obſtructions are not a frequent cauſe of this diſeaſe, is evident from the writings of a late German author, Dr. Richter, who ſays* he has often found aſcites and hydrothorax without the ſmalleſt obſervable fault either of the contents of the thorax, or abdomen; and on the contrary has often found ſuch morbid appearances in the higheſt degree, without the ſmalleſt tendency to this diſeaſe. And if all this be granted, I muſt conclude with the Doctor, that as this diſeaſe diſappears for a time and again returns; is ſometimes preſent in a ſlight, and ſometimes in an alarming degree, accompanied with different ſymptoms at different times; it muſt be incredible that obſtructions in the viſcera, a cauſe which continues conſtantly to act, could ſo vary and change in its effects.

Having, I hope ſatisfactorily proved, that obſtructions of the viſcera, are not a frequent cauſe of dropſy; it is now proper to aſſert that they are not ſo, as they naturally happen, in the human ſyſtem at any time; being always themſelves the effect of

* Page 285.

a morbid action of the blood veſſels, as much as ſuppuration, rupture, or gangrene. But when they do occur, they may from the ſtimulus of diſtenſion greatly haſten the already turbulent motion of the veſſels. An experiment or two taken from Lower and recorded by Dr. Wilkes and others, ſufficiently prove this. "If the jugulars of a dog be tied ſo as to prevent the blood's circulation, he will ſhed an abundance of tears, the ſaliva will flow as faſt from him, as if a ptyaliſm by mercury was raiſed, and a ſerous matter will ooze out betwixt the integuments of the head and the interſtices of the muſcles of the neck. Again, if the vena cava be tied a little above the diaphragm, the abdomen will fill with water, as in an aſcites." Theſe experiments at once prove that effuſion is the effect of an increaſed action of the blood veſſels, and that obſtructions may increaſe this action. The obſtruction here however is very different from thoſe which take place in this diſeaſe; and that too in being mechanical, and in acting upon a healthy ſyſtem, and producing from their indirect ſtimulus, a diſeaſe of exceſſive action of thoſe particular veſſels; whereas the others are only the conſequence of a primary diſeaſe of theſe veſſels, and whoſe action of courſe does not produce a diſeaſe in them; there being an axiom in medicine which aſſures us, that two diſeaſes cannot exiſt in the ſame ſyſtem at the ſame time. Again, that obſtructions are not an immediate cauſe of this diſeaſe, is argued from the cure; in order to effect which ſuch medicines are preſcribed, whoſe known

action is evacuant, and chiefly upon the blood veſſels; and obſtructions as a cauſe are entirely loſt ſight of, and are only noticed as ſymptoms, the conſequent attendants of a morbid ſtate of the blood veſſels.

Dr. Cullen ſays* of all the cauſes of an increaſed effuſion, the moſt frequent and remarkable is, a laxity of the exhalants; and ſince whom it has been ſo univerſally admitted and believed, that it has been the bulwark of all who wiſhed to defend his theory; the compaſs of thoſe who wiſhed to ſteer through this diſeaſe; and alſo the ſtumbling block of all who have failed in its cure. As a laxity of the fibres in general has already been noticed, it will only here be neceſſary to enquire into the cauſe which produces this laxity of the exhalants in particular; and which is moſt frequently a general debility of the whole ſyſtem. Much has indeed been ſaid by moſt authors reſpecting this debility; but from all it muſt be evident, that little elſe than a repetition of opinions, without a proper inveſtigation, has been advanced; and that it has never yet been proved that in any caſe a ſimple direct debility has ever produced this diſeaſe; for Dr. Ruſh has aſſured us in his lectures, from experience that it never took place in thoſe ſtates or conditions of the ſyſtem in which direct debility occurs in the higheſt degree; for ſays he, 1ſt. "in old age this ſtate of the ſyſtem is univerſal, and yet how ſeldom do any old

* Page 258.

people die of dropſies! Nay, how ſeldom do we obſerve even a ſwelling of the feet to take place in old people, who ſit for months and years conſtantly in arm chairs by their fire ſides! 2d. In the laſt ſtage of typhus or low chronic fever, dropſical ſwellings are unknown, where previous evacuations have been uſed, or when they have been unneceſſary. 3d. Dropſical ſwellings ſeldom occur in perſons who die of pulmonary conſumption. 4th. They never occur in caſes of maraſmus and atrophy, in which patients die at laſt from ſimple debility. 5th. And laſtly there is not a ſingle inſtance of a dropſy in thoſe perſons who have ſuffered or died of famine; now in death from this cauſe, direct debility is always in the higheſt degree." But what are the cauſes which can create this debility of the exhalants without affecting the pulſe, which is generally in this diſeaſe frequent, quick and tenſe, and the blood when drawn becomes ſizy? Or why are not the contractile fibres of the arteries relaxed by the ſame cauſes? If they are, then the circulation of the blood muſt be diminiſhed in an exact ratio to that of the exhalants, and of courſe prevent this great and ſudden effuſion. Fevers of all kinds, and intoxicating liquors are ſaid by Dr. Cullen* to be the moſt frequent cauſes of dropſy, and theſe produce the diſeaſe not from their debilitating effects, but their ſtimulating, as muſt be ſenſible to all who feel the pulſe during their action. If therefore, as has been proved, this diſeaſe is not the conſequence of a

* Page 259, vol. iv.

debility of the exhalants, and if the cauſes ſaid to produce this laxity of them, tend rather to excite their action; may it not be fairly inferred, that this effuſion is not produced in the manner juſt inveſtigated? And that this debility, which Dr. Cullen calls the *hydropic* diatheſis, may with equal propriety be changed to the *febrific*, being as conſtant in the one as in the other?

This increaſed effuſion has likewiſe been attributed to a larger quantity of water than uſual being in the ſyſtem. It will therefore be neceſſary to turn the attention to its numerous ſources. It has been ſuppoſed the conſequence of the excretory veſſels not performing their office. That this is a frequent cauſe of this diſeaſe does not appear probable, for the excretories have been affected in the other ſtates of fever, when no ſuch thing appeared in the blood as a greater quantity of water, and no dropſy was the conſequence, and frequently a dropſy has been when the excretories were regular, and when the blood exhibited the ſame appearances as in an inflammatory diſeaſe; and if ſo, this can ſeldom or never be a cauſe.

The quantity of water has been ſaid to be increaſed by blood letting. Never has an opinion been more implicitly believed, and more inviolably held ſacred; and yet when it is examined, it muſt with aſtoniſhment be confeſſed, that this opinion, only becauſe of ancient date, has led men,

called rational creatures, ſo eaſily aſtray. It will therefore be neceſſary to determine if ſuch a thing takes place: and this is generally imagined to be done by its diminiſhing the craſſamentum; but whoever has once ſeen blood drawn muſt acknowledge, that from this apparent homogeneous maſs an equal proportion of ſerum, red globules, or coagulating lymph to their reſpective quantities in the blood, muſt be taken away, and therefore their exact ratio muſt be the ſame after as before a bleeding. For inſtance, if three kinds of fluids in different quantities are intimately mixed and kept ſo by a kind of conſtant circulation, it muſt be evident to all, that if a pint of this mixture be evacuated during its courſe, the remaining quantity will not exiſt in a greater diſproportion than was before the diſcharge. Preciſely ſo is it in blood letting. Dr. Sydenham was an implicit believer in the blood being thinner and weaker in this diſeaſe, ſo great was his faith in the opinion of others; and this is truly ſurpriſing, for he was the firſt who taught us that in diſeaſes of ſevere and inflammatory action, the particles are ſo broken down from the exceſſive action of their blood veſſels as not to unite, and of courſe appear in that form. But ſay ſome, after ven. ſect. the abſorbents may take up the depoſition in the cellular membrane, and convey it into the blood. Although this undeniably happens, yet it does not follow that the blood is neceſſarily more watery; for it is not certain but that as ſoon as it enters the blood veſſels it there may undergo ſuch a change as

to be converted into the proper proportion of the conſtituent diviſions of the blood. And this is certainly probable; for if not, after ſeveral bleedings the blood would ſeem to be moſtly water; but on the contrary, in ſome ſtates of fever, after ten or eleven bleedings, ſo far from there being much ſerum there has not apparently been any at all, the blood being diſſolved.

From what has been advanced it may fairly be doubted whether an exceſſive evacuation, from venſection ever produced a dropſy; for there are innumerable inſtances of this diſeaſe without this evacuation being once uſed, and a number of almoſt incredible loſſes of blood without inducing this diſeaſe. Dr. Ruſh has taken repeatedly by 13, 14, 15 bleedings from 100 to 140 ounces, without producing this diſeaſe. Dr. Phyſick has taken 85 ounces by one bleeding without inducing one of its ſymptoms. Dr. Dover, an Engliſh phyſician, in 1732 bled 180 ſoldiers about 100 ounces at a time, and loſt only 8, in whom he could not prevent the uſe of ardent ſpirits. Botallus not only drew large quantities of blood without any conſequent dropſies, but always bled in them. And Haller* records a number of caſes both in frequency and quantity without the leaſt injury or ill conſequence. The two following are the moſt remarkable. In the firſt, in twenty-four hours 202 pounds of blood was loſt. In the ſecond, a lady was bled 1020 times in

* Elementa Phyſiologiæ, tom. ii.

19 years. If upon the whole, ſuch large quantities of blood have been drawn without cauſing this increaſed quantity of water in the blood, or even creating a laxity of the exhalants, and thus have produced a dropſy, may I not be allowed to conclude, that this diſeaſe is not the production of that invaluable remedy? But ſome may perhaps urge, that although a variety of opinions may exiſt, yet experience has taught that this diſeaſe has ſometimes come on after theſe evacuations. This I do not deny; but it ſhould not be attributed to its exceſſive or injurious application, but rather to its *too ſparing uſe*. For œdematous ancles have often been ſeen after 2 or 3 bleedings, which had entirely vaniſhed upon a repetition of the uſe of the lancet, and health has taken their place; from which it muſt appear, that although ſwelled ancles have been the polar ſtar of ſome phyſicians, and have at once convinced many others of the inevitable conſequence of the lancet, a DROPSY, yet it is in itſelf an ineſtimable remedy, producing it only in the hands of the timorous; and ſwellings are only a ſymptom of a morbid action of the blood veſſels, and are in themſelves as trifling and innocent as a pain in the ſide or breaſt, and always *calling* for a repetition of the remedy. For when phyſicians in common pretend to open a vein, the ſyſtem muſt certainly be raiſed much above the effuſive point; but by 2 or 3 bleedings it is reduced from that higher or inflammatory point down to the effuſive. Not knowing this, they at once ſheathed the lancet, and

the diſeaſe by continuing at this point, produced the diſeaſe in queſtion: whereas had they uſed the lancet a little oftener, they would have reduced the ſyſtem from the effuſive point to the ſimple and leſs dangerous point of morbid action alone, and thereby the former diſeaſe would have been cured without either difficulty, a dropſy, or even a ſtigma to the lancet.

It has been urged, that a watery diet may produce a thinneſs of the blood. This, however, as yet is only an hypotheſis, for Dr. Millman* has ably defended the uſe of a watery diet in this diſeaſe, and aſſured us that the Italian phyſicians threw in large quantities of water in fevers: nor did they fear a dropſy from what they call their *diæta aquea*! And it is well known, that many dropſies have exiſted without ſuch a diet, and in all whom the blood never exhibited ſuch a watery appearance. Beſides, even allowing a watery diet to be uſed, as it is not known what part of our aliment forms the red globules, and what the lymph; and as the chyle appears much the ſame although the production of a variety of aliments, the complete formation of the conſtituent parts of the blood muſt depend not upon the quantity of water, but upon the *appropriate* action of the blood veſſels.

The different methods of an increaſed effuſion have now been noticed, after which I hope it will

* Page 22.

be confeſſed that it is not probable that it happens in either of the ways advanced by Dr. Cullen. But notwithſtanding which I am far from *denying that this effuſion never exiſts*; for I hold, that it always does, and is properly the only manner in which this diſeaſe can take place; and is always the effect of a morbid action of the blood veſſels, produced by the application of an over doſe of ſtimulus, or otherwiſe, a ſtimulus diſproportioned to the excitability or ſtate of the ſyſtem.—And this is more evident from their

CURE.

IT has conſtantly been an object with authors in medicine, to make their cure coincide with their different theories: but as the experience of ages has confirmed the propriety of truth being an unit, in the caſe of no one diſeaſe has it ever been more fully exemplified than in the preſent. For notwithſtanding a variety of different theories and modes of cure have exiſted, yet the trial of them all has been ſo uniformly fatal, that even in our time, this diſeaſe is by many ranked as *incurable*. When a defect of the abſorbents was in vogue, it was found that thoſe medicines which ought to have promiſed a cure were prejudicial; and thoſe which fortunately were ſerviceable, were ſtrangely accounted for: for example, emetics have often in the annals of medicine been recorded a means of cure; and inſtead of attributing this to their proper evacuating

quality, they were ſaid to *promote* the *abſorption*; and ſometimes they have produced a perſpiration, and evacuated greatly thereby; then they were ſaid to *overcome* the *ſpaſm* by tending to the *ſuperficies*. Debility, whether of the fibres of the body or of the exhalants, or in whatever manner produced, has at all times been admitted as the moſt general cauſe of this diſeaſe; and yet the firſt indication here has been to evacuate the water, by medicines whoſe action muſt have debilitated ſtill more, as cathartics, which may be ſeen in Drs. Cullen and Sydenham: when in fact, according to their own theory, their firſt indication ought to have been to ſtrengthen theſe debilitated fibres, by tonics; and when the ſyſtem had become ſtronger, then to diſcharge this water. But how could Sydenham himſelf have preſcribed purgatives, and that too of the moſt draſtic kind, in a diſeaſe of ſuch debility as he ſuppoſed this to be? And yet he aſſures us, theſe promiſed ſucceſs. While this diſeaſe is treated as of debility, if a glance be only caſt at the many ſufferers who daily fall victims to it, it muſt be evident, that not even the yellow fever itſelf has made greater devaſtation. If ſo, the ſolitary inſtances of cure which theſe medicines have effected, muſt be accounted for very differently: for if this diſeaſe be only admitted, a morbid action of the blood veſſels, the operation of all theſe remedies can at once be made plain; for it is certain that purges act by evacuating or depleting from the maſs of fluids in the body, and thereby diminiſh this

morbid action of the blood veſſels: that this is the caſe in the different ſtates of fever, all muſt acknowledge; and preciſely ſuch is their effect in this diſeaſe. Emetics have not only the power of evacuating the contents of the ſtomach upwards, but alſo of diſcharging the fœces copiouſly downwards, and promoting the perſpiration freely, by which it cannot be denied but a large evacuation takes place. And thus likewiſe is the effect of bliſters, diuritics, and a ſalivation; for all of which are undeniably evacuants, only differing in force, and of courſe, whoſe application muſt always reſt upon the ſtate of the ſyſtem or the will of the phyſician.

AS it has been fully proved by the ſymptoms, cauſes, and method of cure, that this diſeaſe depends upon a morbid action of the blood veſſels; it muſt be acknowledged to be as much a FEVER as any other diſeaſe; and that this is really the caſe, its ſimilarity with the other ſtates of fever will more completely eſtabliſh.—And

I. Are fevers moſt commonly the production of *exhalation* from low marſhy countries? So likewiſe is the dropſy. Towne on the Diſeaſes of Barbadoes* obſerves, that before the iſland was rendered leſs moiſt by cultivation, this diſorder was ſo endemic as to be diſtinguiſhed by the title of the

* Page 11.

country diſtemper. Wilkes ſays* this diſeaſe is very common in fenny places, and is owing to the atmoſphere being overloaded with noxious vapours. And Dr. Richter† aſks, if experience does not teach us that dropſy is much more frequent in damp marſhy countries, and there more difficult to cure than in dry ones: and that in warm climates this diſeaſe frequently diſappears of itſelf. In point he relates the caſe of "an Italian merchant who ſat at an ordinary in Holland, eating with a good appetite, and was all over dropſical. Upon enquiring into the ſtate of his health, and adviſing him not to neglect his diſeaſe, was anſwered, that he was convinced his diſeaſe was of but little conſequence, for he had already been five times in Holland, and had at each time got the dropſy, which had always gone off, as ſoon as he had arrived at his home in Italy." Moſt authors have noticed the frequency of this diſeaſe in moiſt cold weather, and in wet ſituations. That this is the effect of marſh effluvia no one will deny; for it has on all hands been allowed to be its moſt fruitful ſource. How this acts, whether as a ſedative or a ſtimulant, or whether it is abſorbed by the pores of the ſkin, or received into the lungs, and there undergo a change, has for a length of time been diſputed. That it is a ſtimulant, I take for granted, in conſequence of its producing a fever, which I hold to

* Page 320.
† Page 291.

be the effect of an irregular or morbid action of the blood veſſels, and produced by a ſtimulus: and becauſe the Profeſſor of the Inſtitutes aſſures us, its action is direct, ſtimulating the blood veſſels, and creating many diſeaſes of an inflammatory nature, as fevers of moſt kinds, as the bilious, remitting, and dyſenteric. As to an abſorption, I ſhall not here pretend to ſay but that ſuch a thing may poſſibly happen; yet I cannot imagine it probable that this diſeaſe is produced thereby, for the body is moſt commonly ſo well clad as to prevent it in ſuch a degree as muſt be requiſite to create it; and becauſe there are not any proofs of ſuch a thing taking place in any of thoſe who have had this diſeaſe. And more eſpecially, becauſe it can be more eaſily accounted for by imagining the lungs to be the channel through which exhalation is received to produce its effects upon the ſyſtem. Although I do not pretend to decide in what conſtituent part of the exhalation its injurious quality may reſide, yet may it not, be it in what it will, be in a greater proportion in a given quantity of ſome ſtates of the atmoſphere, than in others, and of courſe may be taken in the lungs in a larger doſe, and thus raiſe the ſtate of the ſyſtem, by its ſtimulating property, from the *healthy* to the *effuſive* point, and produce this diſeaſe? And this ſeems probable, for Chaptal* aſſerts, that vital air in reſpiration paſſes continually from the gaſeous to the concrete ſtate;

* In his Chemiſtry, vol. i. page 137.

it muſt therefore at each inſtant abandon the heat which held it in ſolution, and in the ſtate of gas." Shortly after he ſays, "hence it follows that during the winter, the heat produced muſt be more conſiderable, becauſe the air is more condenſed and exhibits more vital air under the ſame volume."— In like manner exhalation in conſequence of the abundance of caloric in dry and hot weather, muſt be kept diffuſed, like vital air, in a larger volume of the atmoſphere, and muſt be in a more complete gaſeous ſtate; and if ſo, muſt be breathed in a leſs quantity at every act of inſpiration, and of courſe muſt produce a leſs injurious effect than when the atmoſphere is colder, more moiſt, and condenſed. And that this is really ſo, the following fact cannot but prove. Dr. Ruſh informed me that a gentleman in Maryland about to return home in the evening from a neighbouring viſit, regretted that he had to paſs by a place which always about that time ſent forth a very diſagreeable fœtor. His friend preſſed him to remain until midnight, when he aſſured him, this offenſive ſwell would not be perceived; he did ſo, and the event was preciſely as predicted. This important fact in itſelf evinces the propriety of this opinion; for the action of the ſun being withdrawn, the caloric became more latent, and the atmoſphere was ſo condenſed as to concentrate the exhalation in that portion of the air which was within his height, and which of courſe acted ſo forcibly upon the organ of ſmell, and which gave ſuch uneaſineſs in reſpiration; but

at midnight the atmoſphere was not only concentrated as much as poſſible, but the exhalation muſt have been precipitated to the earth, and thus have left the ſurrounding air perfectly ſweet. If all this be acknowledged it muſt be confeſſed that EXHALATION acts in this diſeaſe as in other ſtates of fever, and in all it produces a morbid excitement of the blood veſſels. This indeed ſeems to be the caſe, for this complaint is moſt prevalent in wet ſituations, intermittents in thoſe leſs ſo, dyſenteries in thoſe the leaſt ſo, and when neither of theſe exiſt, and where the ſun and air have a free acceſs, diſeaſes ceaſe to be an evil to mankind. Some may perhaps object to this mode of explanation, becauſe in ſome countries where as much water falls in one year, as in any one place, this diſeaſe is ſcarcely known. This I can eaſily conceive may be the caſe, and yet not prove a powerful objection; for Dr. Ruſh in his lectures has very properly taught us, that the action of marſh effluvia in a great meaſure depends upon the ſituation of the ground, and its moiſture. If the land be level and completely overflowed, the exhalation is trifling and not injurious, and thus he ſays Mr. Bruce in his travels remarked that the rainy ſeaſons, which perfectly covered the low grounds, were never unhealthy, in one of the ſickly countries which he viſited. But in ſome others which have no moraſſes, and are more hilly, Daziller obſerved* the rainy ſeaſons to

* In account of Diſ. Neg.

be the reverſe. And that a great fall of rain may happen in a place and not produce this diſeaſe, is very probable; for I can eaſily believe that ſo great a quantity may at once fall as not only to check, but even prevent for a time any exhalation. This in a ſhort ſpace of time however muſt ceaſe to act, and would be the very inſtrument of its generation if a ſucceeding ſhower or two, did not ſo diminiſh it as to prevent that quantity to be generated at any one time as is requiſite to create febrile diſeaſes. This is generally obſerved in our common epidemics, which are always prevented from being fatal by ſucceeding and refreſhing ſhowers. If this proof is not ſufficient, I truſt the following, from Huxham†, will place it beyond a doubt. In Auguſt 1735, the month was ſo remarkably wet, that every place was filled with mud and water. The birds died in moulting. The leaves of the trees fell as if it had been in the midſt of winter. And in all this month not a ſingle dropſy is mentioned by Huxham as one of the diſeaſes. In September the weather continued juſt the ſame; and among its diſeaſes there was not the leaſt veſtige of a dropſy. But in October the ſeaſon was not ſo wet, and in this month intermitting and *aſthmatic* fevers, as he called them, terminated in a dropſy of the abdomen and feet. This is now in itſelf ſo evident that I ſhall at once decide that a dropſy is as much a fever as the intermittent or other ſtates; and that in ALL, this exhalation is the remote cauſe, producing

† On Epidemics.

by its ſtimulus an irregular or convulſed action of the blood veſſels.

II. Do the other ſtates of fever occur at the ſame ſeaſon? In like manner does the hydropic ſtate. That the different ſtates of fever do occur at the ſame time, muſt be familiar to every one; and that the dropſy frequently exiſts at the ſame period with other diſeaſes of the arterial ſyſtem, all phyſicians and authors of experience have admitted. Dr. Richter* ſays, in the autumn of 1791, when almoſt all diſeaſes were of the rheumatic kind, dropſical ſymptoms accompanied them. Huxham ſays, in 1747 there was a putrid fever which frequently was attended with a ſwelling of the abdomen. And that it ſhould exiſt at the time of a rheumatiſm, or pneumony, is indeed now not very ſurpriſing; for it has been, I hope, incontrovertibly proved to be of the ſame ſyſtem, produced by ſimilar cauſes, which create the ſame irregular action of the blood veſſels.

III. Have all the other ſtates of fever debility for their prediſpoſition? Such alſo is the caſe in the hydropic. The term debility has moſt undoubtedly ſlain its thouſands; for while all have admitted it as an object in the cure of diſeaſes, few or none have underſtood its nature; moſt of them, ranking, as of that order, all diſeaſes in which the patient either looks *pale* or feels *weak*, or the *na-*

* Page 291.

ture of which they do not comprehend. And never before the time of Dr. Brown, were it taught to be of two kinds, *direct* and *indirect*. This diſtinction when kept in view, cannot for its uſefulneſs, but reflect the higheſt honour upon its author; yet unfortunately, he himſelf loſt ſight of it, when he aſſured us, that although ariſing from either an abſtraction or exceſs of ſtimulus its cure muſt be effected in the ſame manner. This inconſiſtency and contradiction however muſt become harmleſs, when it is recollected, that the *debility* ariſing from *intoxication* and *famine* muſt be remedied very differently. The Profeſſor of the Inſtitutes has in his lectures not only taught the propriety of this diſtinction, but in the IV. Vol. of his Enquiries has moſt ably and ſucceſsfully proved them to be in themſelves not a diſeaſe, but only a prediſpoſing or inviting cauſe. And that it is preciſely ſuch in dropſy, the following fact, which came within my own knowledge, will ſufficiently teſtify. A gentleman was ſuddenly reduced from a common and healthy diet, by confinement, to bread and water; in this ſituation he remained for months, and enjoyed his health without the leaſt ſymptom of diſeaſe. After this, his regimen was ſuddenly altered, and a more ſtimulating and nouriſhing one allowed; when lo! in three or four days an inveterate dropſy was the conſequence, which took three bleedings, in each of which the blood was ſizy, and a ſalivation to ſubdue it. This inſtance at once evinces that *debility* ſimply is not in

itſelf, a diſeaſe, nor the immediate cauſe of dropſy, but only an inviting cauſe. For by the abſtraction of theſe ſtimuli the excitability was ſo much increaſed as to enable the moderate ones, of bread and water, to keep the ſyſtem to the healthy point; but the more nouriſhing diet immediately raiſed the ſyſtem from the healthy to the effuſive point: and thus a dropſy was the conſequence, which now muſt undoubtedly be admitted, the *effect* of a morbid action of the blood veſſels.

IV. Are particular conſtitutions more liable to ſome ſtates of fever than to others? Preciſely the ſame is it with the hydropic fever. Moſt authors have remarked that men are not ſo ſubject to it, nor ſo difficultly cured, as women; and that children are eaſier relieved than either. It is not uncommon to obſerve ſome families ſubject to the arthritic, others to the apoplectic, and others the hermorrhoidal ſtates; ſo likewiſe Hippocrates* has noticed this hereditary diſpoſition in the dropſy: And Dr. Wilkes† had a female relation who had been ſubject to it from her infancy, and who at the age of 50 died of it, and whoſe mother not only labboured under it when with child of her, but alſo fell a victim to it. And Huxham, in lamenting the loſs of his conſort,‡ ſays "ſhe deſcended alas, from an hydropic race." Shall I now heſitate in declaring this diſeaſe to be of the ſame nature as a fever?

* Lib. prædict. ii. page 89.
† Page 124.
‡ Page *24.

V. Do the other ſtates of fever attack all ages and both ſexes? So alſo does the dropſy. And that this is really the caſe, the experience of all practitioners muſt determine.

VI. Do the different ſtates of fever alternate with each other? Such undoubtedly is the caſe in the hydropic ſtate. Dr. Ruſh ſays* he has ſeen anaſarca alternate with vertigo, and both aſcites and anaſarca with tonic madneſs. Dr. Darwin† mentions two caſes of dropſy having a temporary cure from inſanity. In the rheumatiſm it ſeldom happens, ſays Dr. Cullen,‡ that a ſwelling coming on does not alleviate the pain of the joint: and Dr. Richter§, mentions a violent caſe of rheumatic head-ach, which ſuddenly vaniſhed after the feet ſwelled; in ſeven days after, the head-ach returned, and the ſwelling diſappeared. Alſo§ of a dropſy from an ill cured itch, the patient was inoculated with itchy matter, and the diſeaſe had ſcarcely made its appearance, when the dropſy diſappeared. But of the many facts related by that elegant author, the two following are the moſt important. In the one§ "the patient had firſt a catarrh, then pains in the knees, which were ſo violent that he could not move; ſoon after his abdomen and face ſwelled; the pains in his arms and breaſt were increaſed, until an expectoration of a

* Vol. ii. page 167.
‡ Page 15, vol. ii.
|| Page 295.
† Sect. 34. page 435.
§ Page 291.
¶ Page 318.

purulent colour, which was ſometimes ſtreaked with blood, took place: after this his dropſy abated, and he was now violently attacked with a ſtitch in his breaſt, with a full, quick, and tenſe pulſe. The dropſy now quite gone, but the pains more violent, ſometimes in the knees, and ſometimes in the arms, attended with a fever. The ſwellings of the legs returned, which gave ſome relief to his pains. Theſe ſwellings were now attended by a little inflammation, which, however, went off with a copious perſpiration, and the patient was ſhortly after freed from every complaint." In the other,‖ "ſometimes the neck alone ſwelled, ſometimes the face, ſometimes the arm, and ſometimes the feet. Twice the patient had all the ſymptoms of the hydrothorax, and once a dropſy of the lungs. He had aſcites repeatedly, and all theſe ſwellings changed and took place ſo ſuddenly, that in the evening there was not the leaſt appearance of that ſwelling which in the morning was prodigiouſly large."— Jackſon ſays,* dyſentery and dropſy frequently made their appearance in the months of Auguſt, September, and October, alternating or ſucceeding the intermittent. And in another place†, he ſays, "at Camden, South Carolina, the intermittent, the dyſentery, and dropſical ſwellings ſo often alternated with one another, as evidently ſhewed that they all

* Page 197.

† In his Treatiſe on the Intermitting Fever of North America, page 199 and 200.

‖ Page 198.

depended upon the ſame general cauſe." That ſome ſtates of fever ſhould alternate with others, is no thing remarkable; but that a dropſy ſhould, has appeared wonderful; this, however, will no longer be ſo, when it is recollected, that all thoſe diſeaſes which do this are primarily of the ſame ſyſtem; for no two diſeaſes can exiſt at the ſame time in the ſame ſyſtem, as the ſtimulus producing the one muſt be ſtronger than the other, and of courſe deſtroy the *one morbid* action by creating a *different* and *greater* action. Upon this principle hangs the ſolution why dropſy alternates with theſe other ſtates of fever.—And as an additional proof

VII. Do the other ſtates of fever terminate in each other? So likewiſe does the hydropic ſtate. Every practitioner muſt have witneſſed this in the other diſeaſes; and that this is really the caſe in the dropſy, the following facts muſt inevitably prove. It is not uncommon to ſee this diſeaſe terminating in the form of a diarrhœa; this has been noticed from the time of Hippocrates to the preſent period: as is ſufficiently confirmed in the writings of Hippocrates, Ferriar's Medical Eſſays, and Wilke's Treatiſe on the dropſy. Dr. Cullen* ſays he has remarked a fatal hydrothorax, and there are many inſtances in which a ſpitting of blood came on ſeveral days before the patient died. Wilkes records† a caſe which terminated in an epiſtaxis.

* Page 317. vol. iv.
† Page 48.

Mr. Wiſeman ſays* he was conſulted for a prolapſus ani; upon obſerving the ſheet upon which the perſon was, much wetted, he concluded he was dropſical; the patient died apoplectic, and upon diſſection, much water was found in the abdomen. Dr. Cullen† ſays it is not unuſual for a general dropſy to end in apoplexy.—As a further evidence of the ſameneſs of this with the other ſtates of fever

VIII. Are ſome ſtates of fever the entire conſequence of others? And ſuch is truly the caſe in the hydropic. That this happens in the other ſtates needs no demonſtration. Wilkes‡ has known a colic to produce an aſcites: and in the Medical Commentaries§ is recorded a ſimilar inſtance in conſequence of a bilious colic, and which terminated fatally after 928 pints of water had been diſcharged in 15 months by 25 different operations. In the IV. volume|| is recorded a dropſy from the angina maligna. Dr. Cullen¶ ſays there are inſtances of pneumonia terminating in hydrothorax. Alſo* that aſthma, after a long continuance, often ends in it. And Huxham in 1746 records a ſevere pleuriſy which when ever it ended fatally, death was always foretold by watery ſwellings of the feet. That dropſy is the conſequence of remittent and intermittent ſtates of fever, Lind, Pringle, and moſt authors affirm. Dr. Richter* aſks, how many exam-

* Book iii. chap. 2.
‡ Page 45.
|| Page 75.
* Page 400 vol. iii.
† Page 145, vol. iii.
§ Vol. iii. pags 503.
¶ Page 241, vol. i.
† Page 290.

ples are there of dropſy being the effect of the itch improperly cured; of eruptions of the ſkin which had gone in, of the venereal poiſon, of the ſcarlet fever, of rheumatiſm, and of gout? And if all this be acknowledged, and as moſt of them are known to be diſeaſes of a morbid excitement of the arterial ſyſtem, it muſt alſo be admitted, that the dropſy, as well as all thoſe muſt depend upon a morbid ſtate of the blood veſſels.

IX. Have all the other ſtates of fever two ſtages, acute and chronic? So alſo has the hydropic. That this is ſo in fevers in general, will undoubtedly be admitted: and that there are alſo two ſtates in the dropſy, no one has ever denied; this indeed has always been acknowledged. Dr. Ruſh, in his II. volume of Enquiries, not only allowed them, but has called them the *tonic* and *atonic*: the latter to be known by occurring in habits naturally weak. With all deference, I ſhall here preſer the terms *acute* and *chronic*; and that too, 1ſt. Becauſe tonic ſuppoſes the ſtate always to have what has been called a phlogiſtic diatheſis, and muſt be of ſo inflammatory a nature as at all times to require depleting remedies; which is not the caſe. Whereas the *acute* only aſſures us, the diſeaſe muſt terminate in a ſhort time, as all other diſeaſes of that nature: and though generally of a violent morbid action, it does not induce any one to ſuppoſe it muſt *always* be ſo; for its morbid action may be ſo ſlight as to be reduced by moderate evacuations, or even to be overcome, as in the other ſtates of fe-

ver, by a tolerable ſtimulus, which might create in the ſyſtem a greater and, poſſibly, a different action. 2. I ſhall uſe the term *chronic* becauſe it ſuppoſes the diſeaſe to be of ſome ſtanding, by which its morbid excitement muſt be greatly diminiſhed; and conſequently its excitability muſt be conſiderably waſted; whereas *atonic* conveys an idea of weakneſs from the commencement, in which the excitability muſt be increaſed. Now this we know not to be the caſe; for if the excitability were increaſed a moderate ſtimulus would have effect in curing this chronic ſtate, and that too becauſe it is an invariable law of the animal economy, that the action of a ſtimulus will be more powerful as the excitability is increaſed; but on the contrary, experience daily teaches that in the *chronic*, the moſt powerful ſtimulants are requiſite, and that too becauſe there is another law which alſo aſſures us that to create a new action the ſtimulus muſt be greater than that which already occupied the moſt of the excitability of the ſyſtem: and this is known to be preciſely the caſe not only in this ſtate of dropſy but in all chronic diſeaſes. Beſides, it will often be neceſſary to reduce the ſyſtem in the chronic by evacuants, in order to diminiſh ſtill more the morbid excitement, that theſe ſtimulants may operate more certainly and expeditiouſly. 3. I prefer the term chronic, becauſe Dr. Ruſh has very properly taught us that the weaker the ſubjects are, not only more liable muſt they be to diſeaſes of great morbid action, but the

more requiſite muſt evacuants be to reſcue the debilitated veſſels from rupture.

X. Have ſome ſtates of fever different kinds of effuſion? So alſo has the hydropic. Every perſon the leaſt converſant with this diſeaſe, will confeſs that this is the caſe; for not only ſerum and blood, but effuſions of all colours and conſiſtencies have at different times been diſcovered: But that is not all, even hair, bone, and teeth have been found upon diſſection; inſtances of which may be ſeen in Wilkes: and I have been told that Mr. J. Hunter uſed to exhibit a diſeaſed rectum, in which a tooth had grown. Theſe curious and wonderful phenomena have at all times remained unexplained. Can they not be accounted for by admitting, that as it is now pretty evident, that an effuſion is the effect of a diſeaſe of the blood veſſels, and as the different parts of the body are both formed and nouriſhed by them, and as Dr. Ruſh has in his IV. volume of Enquiries made a miſplaced ſtate of fever, may not where theſe phenomena happen, not only a *miſplaced*, but a *miſeffuſed* action exiſt in thoſe particular veſſels?

XI. In the other ſtates of fever have diſſections diſcovered the ſame morbid phenomena? Such alſo has been the caſe in the hydropic ſtate. It is not by any means a very uncommon circumſtance in all the different ſtates of fever to find inflammations, adheſions, and pus, all the entire effect of a violent action of the blood veſſels. So likewiſe in thoſe of dropſical ſubjects are the ſame appearances. Fer-

riar has given a number of diſſections, moſt of which exhibited ſuch marks. So likewiſe has Dr. Millman* noticed ſuch; as is alſo recorded in the Medical Commentaries†. And Morgagni is overſtocked with them; and the many diſſections in the Pennſylvania Hoſpital for a year or two ſince have confirmed the truth of all former ones, to which I beg leave to refer. Beſides, its ſimilarity with the hydrocephalic ſtate of fever which has been proved by Drs. Quin and Ruſh as of exceſſive excitement at once ſhews that it muſt depend on ſtimulus acting on the blood veſſels, and there producing ſuch an action as to create theſe ſame morbid phenomena.

XII. In the other ſtates of fever, does the blood ſhew a morbid appearance? Such preciſely does it in the hydropic ſtate of fever. That this is the caſe ſome very reſpectable authors have witneſſed. Dr. Ruſh ſays* it has been noticed by many writers. Dr. Millman† bled with great relief, and found it ſizy. Dr. Wilkes not only experienced the truth of this, but even ſays, if blood could be drawn in this diſeaſe, without a hazard of reputation, it would always be found ſizy. And Botallus bled in this diſeaſe without the leaſt inconvenience; and I dare ſay with good effect, for I have often ſeen blood drawn in this diſeaſe as ſizy as in the ſevereſt pleuriſy. Theſe inſtances are enough to convince

* Page 46.
† Page 503. vol. iii.
‡ Vol. ii.
§ Medical Pamphlets, page 98 & 106.

all, of the UNITY of *dropſy* and *fever*, and are ſufficient to eſtabliſh the propriety of bleeding in this diſeaſe, if no others could be adduced. But there are others, for

XIII. In the other ſtates of fever, is the pulſe always in a morbid ſtate? Such alſo is it in the hydropic ſtate. This many phyſicians have noticed, and all have united in declaring the preſence of a morbid action of the blood veſſels. Dr. Ruſh in his lectures aſſures us, it is full, quick, tenſe, and ſometimes ſlow, as in the other ſtates of fever. Dr. Millman has alſo found it full, quick, and ſometimes ſlow. And the experience of all who will examine it, muſt confirm it to be as in the other ſtates of fever. It is indeed much to be lamented, that pyſicians have ſo long neglected the pulſe in this diſeaſe: this being always ſuperceded by the more prominent abdomen and palid countenance; and which have for ages, been a general and ſufficient evidence of a great weakneſs, and want of blood. And when indeed blood has been taken, it was always in the moſt ſparing manner, and never before an *excrutiating pain* in the ſide or breaſt had loudly *called* for it; called I ſaid, for without it, were the blood veſſels ready to burſt, the lancet would remain ſheathed; but with it, even were the morbid action ſo ſlight as to be overcome by a ſtimulant, *bleeding* would not only be faſhionable, but might be performed at all times without the leaſt hazard of reputation. Such is

the effect of prescribing for the name of a disease! Whereas, was this disease considered as a FEVER, as it really is, we would attend not to the appearance of any one particular symptom, nor would we prescribe for the name, but would at all times be governed by the state of the system, which could always be discovered by the pulse.

WHOEVER examines impartially what has been advanced in favour of this disease being of the same nature as fever in general, must acknowledge, that as it has been supported, and I hope established, from facts, as arising from the same causes, produced in the same manner, and affecting the same system; that its

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must of course be the same, and is an irregular or convulsive action of the arterial system. And if all this be admitted, its CURE must undoubtedly be conducted in the same manner; always bearing in mind, that the remedies are ever to be adapted to the STATE OF THE SYSTEM.

FINIS.